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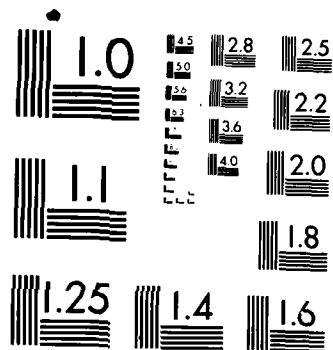
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A RAND NOTE

CONCEPTS, PROBLEMS, AND OPPORTUNITIES FOR USE
OF ANNIHILATION ENERGY: AN ANNOTATED BRIEFING
ON NEAR-TERM RDT&E TO ASSESS FEASIBILITY

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see reverse side

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This Note discusses several issues inherent
in exploiting the energy released when
matter and antimatter annihilate. It
reviews some of the fundamental
difficulties in producing antimatter and
means for storing it. If these
difficulties have satisfactory solutions, a
number of applications for antimatter are
likely to emerge. The Note emphasizes the
fundamental importance of the very large
classes of interesting research efforts
underlying applications goals, and the
anticipated rapid growth of science needs
for antimatter at low energies. The author
suggests a near-term program to resolve the
current uncertainties of suitable
production and storage technologies, so
that basic feasibility of applications of
antimatter can be decided, and decisions on
whether to undertake expanded production
with appropriate storage can be objectively
made.

PREFACE

Under Air Force auspices, in the summer of 1983 Rand examined the possibilities for exploiting the high energy release resulting from matter-antimatter annihilation. The resultant briefing notes and additional documentation were widely distributed in the fall of that year.

Although one can be skeptical of realizing near-term practical embodiments for using annihilation energies, well-defined steps (analysis and experiment) can lead to an early and higher confidence resolution of uncertain utilization issues, at relatively low cost. It has been Rand's view that these steps are worth taking. Possible outcomes might range from (a) a finding that the implementation difficulties are so severe as to make it fruitless at any near time to pursue the exploitation of annihilation energy release to (b) a finding that certain approaches are worth pursuing in a carefully posed RDT&E program, to achieve useful applications goals at an acceptable near time.

The Note, prepared for Project AIR FORCE under a concept development project in the Technology Applications Program, with additional support from Rand's own research funds, focuses on some RDT&E problems that need to be addressed to resolve or reduce uncertainties. Most of the basic scientific issues are not explored here at length; however, a detailed reference list is appended for the interested reader. The Note emphasizes the fundamental importance of the very large classes of interesting research efforts underlying applications goals, and the anticipated rapid growth of science needs for antimatter at low energies. Two major planned experiments, by teams headed by the University of Washington and by the Los Alamos National Laboratory, reflect a portion of the fast evolution of scientific interest in the United States and Europe.

Much of the material in this Note emphasizing the need for carefully posed RDT&E programs was first presented to a small review committee chaired by Dr. Keith Brueckner (University of California at San Diego) in June 1984.

SUMMARY

This Note discusses, in a largely nontechnical way, several issues inherent in exploiting the energy released when matter and antimatter annihilate. Some of the fundamental difficulties in producing antimatter and means for storing it are reviewed. If these difficulties have satisfactory solutions, a number of applications for antimatter are likely to emerge.

The point of view of the Note is that current uncertainties in the basic understanding of problems of suitable production and storage do not permit confident assertion that these technologies can be developed, in a reasonably near time, to any widespread applications. Similarly, it is not possible to prove demonstrably that solutions to these problems will not be achievable in a reasonably near time, although it is clear that any solutions will be difficult and complex.

We believe a well-defined analysis and experiment program can be formulated which seeks to resolve these current uncertainties at a pace likely to surprise many. Solutions, time scales, and the promise of being able to use antimatter can then be assessed with much higher confidence. The Note therefore emphasizes RDT&E programs in physics and engineering which can lead to higher confidence assessments and remove many uncertainties.

There is an enormous amount of intrinsic pure science inherent in this RDT&E which should draw creative scientists to the field.
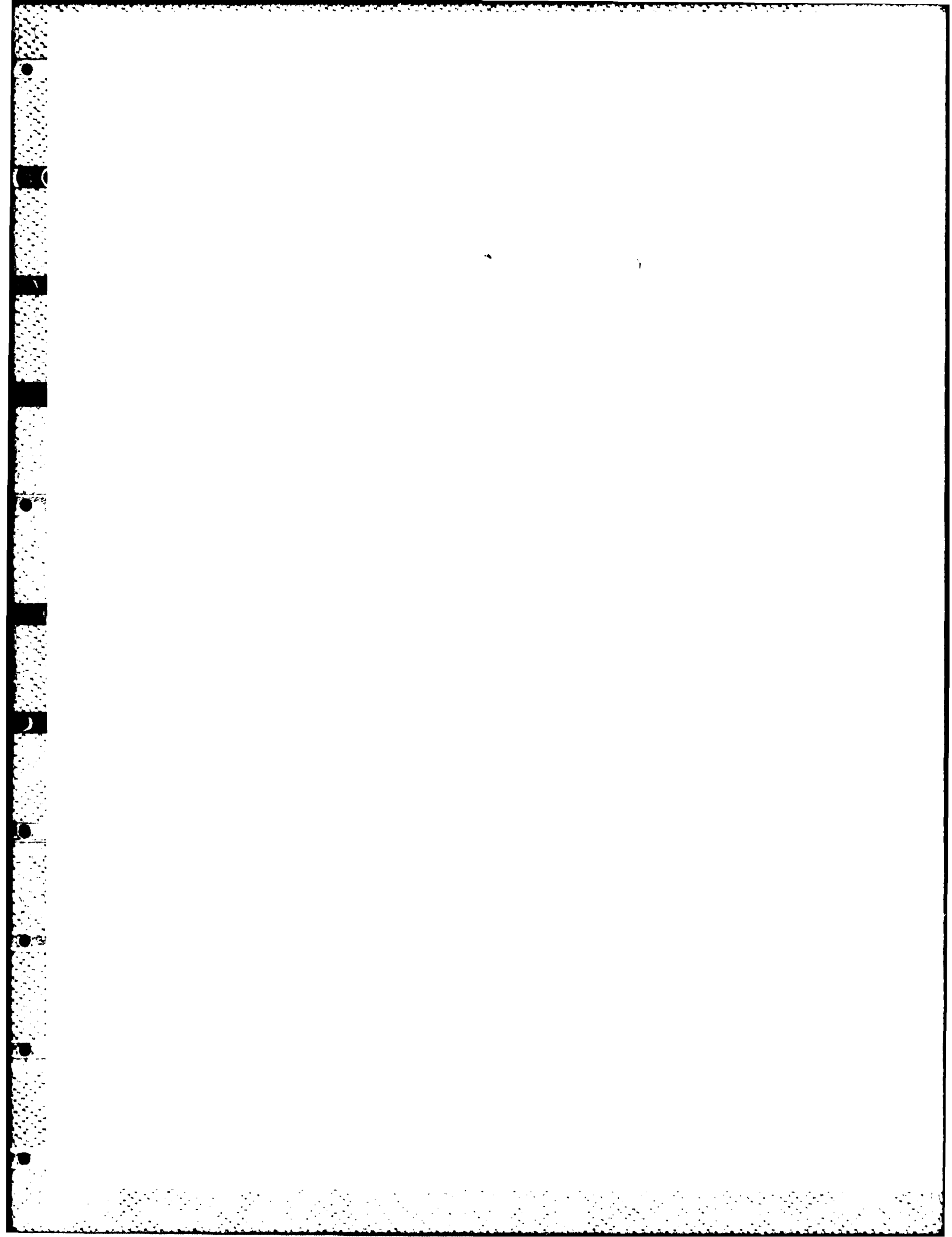
Vital, extremely important precursors to hands-on work with antimatter are normal matter experimental counterparts. These experiments, along with experiments handling present technology levels of $\sim 10^8$/sec, $\sim 10^{13}$ total antiprotons, and transportable antiproton reservoirs, would decide many crucial feasibility questions within about 5 years.

ACKNOWLEDGMENTS

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AN ANNOTATED BRIEFING

OUTLINE OF DISCUSSION

● Background; where we are

● Key problems
  – Production issues; product cost estimates
  – Compact storage

● Sample of basic research, experiment programs
  – A very rich menu exists
  – Performable in academic, industrial, national
    lab settings

● Illustration of systems issues

● Development program example

● Summary

The outline of the discussion shows the context in which we discuss the central issues of annihilation energy. Some of the problems involved are reviewed in terms of potential systems payoffs. That is: If we could overcome some of the evident difficulties of utilizing annihilation energies, would there be worthwhile benefits to such utilization? We will note later that some care needs to be taken in these discussions--there are treatments in which it is easy to ignore some of the essential physics in specific areas and come up with a faulty perspective of possible payoffs. A case in point arises in the discussion of intranuclear absorption and its relevance in calculations of the amount of localized energy deposition taking place when annihilations occur in normal matter targets (page 38 et seq.)

WHY CONSIDER ANTIMATTER USE?

- Energy release from matter — antimatter annihilation
  - Highest attainable ($= 2 mc^2$)
  - Effective energy density ~ 44 kilotons/gram antimatter
  - Normal matter provides 1/2 energy release in annihilation
  - Concentrate on heavy, stable, charged particle (antiproton)
  - Antiproton/proton, antiproton/neutron annihilation
    → ~ 1.9 GeV energy release

- Antimatter has special problems
  - Has to be manufactured
  - Manufacture is energy intensive
  - Needs to be stored without contacting material walls
  - Storage should be long term, light weight
  - Expensive product, but can make economic, systems sense to use

- Physics, engineering interest
  - Unique phenomenologies
  - A great many physics, engineering areas impacted
  - Very high potential payoffs

- No guarantee of ultimate success in large scale applications of antimatter
  - But: near term RDT&E paths to more confident assessment
  - Near term RDT&E to gauge probable success prudent, not very expensive

Use of annihilation energies is driven on the one hand by the very high energy density available in principle, and on the other hand by the special problems posed by antimatter.

It is easy and often customary to be dismissive *a priori* of annihilation energy utilization as too difficult or too remote, or both, to warrant serious consideration for any operational application. A more prudent reaction is to realize that neither skeptics nor enthusiasts can today confidently support their asserted positions, and that as a consequence objective assessment is needed and possible.

In the USSR a reasonable, cautious, and balanced position on the problems and utilization of antimatter is taken, as the following quote from the most widely used undergraduate-level nuclear physics text indicates:

> It may easily be shown that only 0.1-0.3% of the rest masses of the nuclei taking part in a reaction is liberated in the form of energy in fission or fusion. A natural question arises of whether a more efficient liberation of the rest energy Mc^2 is possible. To this end the nucleons must transmute into lighter particles--pions, leptons, photons. But the disintegration of nucleons is strictly prohibited by the baryonic charge conservation law.

> However no conservation laws forbid the liberation of the rest energy of the nucleons in the process of annihilation of matter with antimatter consisting of antinucleons and positrons. The specific power yields in case of annihilation would exceed the yields of the existing power plants by two or three orders of magnitude. But antimatter does not exist in nature, at least in the region of the universe nearest to us. The production of antimatter is feasible in principle, but it will be very costly and will consume energy substantially exceeding the energy of annihilation. Therefore annihilation cannot be a large-scale source of energy. The use of annihilation power might be possible in the remote future for the propulsion of ultralongrange spacecraft.

> Y. M. Shirokov and N. P. Yudin, "Nuclear Power," *Nuclear Physics*, Vol. 2, 1982, pp. 140-141.

ANTIMATTER ECONOMICS — CONSERVATIVE VIEW

- **When use antimatter?**
  - /Cost antimatter production, handling/ < /cost savings by use/

- **Current high energy physics use example**
  - Colliding beam technology
  - Cost: one accelerator + antimatter production < cost: two accelerator systems

- **Space platform use**
  - Platform mass major cost determinant
  - Reducing platform mass — high leverage driver for antimatter use

- **Platform fabrication + launch costs: LEO example**
  - Fuels, fluids, tanks, pumps, lines, valves $\sim$\$5-10$\times10^6$/mt $\left.\begin{array}{l} \end{array}\right\}$ "average" perhaps
  - Payloads, other dry hardware $\sim$\$20-40$\times10^6$/mt $\qquad$ \$15-25$\times10^6$/mt

- **LEO platform application questions to ask**
  - Use of X amount of antimatter reduces platform mass by n mt?
  - Cost of X amount < $\sim$\$15-25$\times10^6\times$N?

The cost of producing antimatter will (with known techniques) be high; specific estimates will be given subsequently. When or whether to use annihilation energies will then in very important respects be an economic issue. That economic issue has two aspects:

- Cases where annihilation energy simply replaces other available alternatives (the "conservative view").
- Cases where no alternative is available and/or practical. These cases likely include effective interstellar flight technologies, for example.

In this Note we focus largely on the conservative view. The specific systems example treated consequently considers replacement of certain functions, which could be done in other ways, on a space vehicle. Here there are identifiable circumstances where the use of annihilation energies can in principle save significant platform mass. The cost of fabricating and launching into orbit this conventional platform saved mass can then be compared to the cost of producing, storing and handling the requisite antimatter to perform comparable missions. If the cost of the latter is below the cost of the former, favorable circumstances for use of antimatter exist.

CURRENT STATUS — ANTIMATTER BASIC PHYSICS, ENGINEERING

DONE/PLANNED WITH ANTIMATTER (CERN; OTHERS)

SOME OF THE ANTIMATTER BASIC KNOWLEDGE NEEDED FOR APPLICATIONS ASSESSMENTS

DONE/DOABLE WITH NORMAL MATTER

1. $\bar{P}$ generation (antideuterons also observed)
2. $\bar{P}$ collection; accumulator design
3. $\bar{P}$ cooling, deceleration stages
4. $\bar{P}$ cooling, deceleration to subrelativistic velocities
5. $\bar{P}$, e^+ conversion to $\bar{H}$
6. $\bar{P}$ very cold, very slow beams
7. Production of antimatter exotic $\bar{P}$ atoms
8. Storage: in storage rings ($\sim$10 MeV) in small electromagnetic traps (few eV to few KeV)
9. Manipulation into, out of storage (process losses, etc.)
10. Annihilation phenomenology at low kinetic energies (including in heavy/fissile elements)

11. Guiding, stopping, trapping of antihydrogen
12. Conversion to condensed states
13. Long term storage of condensed states
14. Extraction, manipulation of constituents
15. Storage of spin polarized forms
16. Storage in special sites in normal matter
17. Test of specialized accumulator and storage designs
18. Test of variant cooling schemes

(some proposals exist)

Crucial Initial Experiments Performable with Normal Matter

If we take a snapshot of where we are in important aspects of using antimatter, some form of this chart results. For many proposed uses of antimatter, further critical experiments are clearly relevant (those, for example, below the horizontal dotted line).

The important point to observe is that in essentially all circumstances these critical further antimatter experiments can be first performed with normal matter (the few specific exceptions are easily identified).

Two conclusions result:

- A great deal of the critical experimental work, particularly in storage, can be done in conventional laboratory settings, and need not initially require access to the very few facilities now capable of producing, e.g., antiprotons.
- This critical experimental work spans present disciplines such as atomic and molecular physics, condensed matter physics, the physics and chemistry of solid state, etc. Many current experimental techniques are directly applicable.

Our position is that this critical experimental work, which is identified in further detail on page 29 et seq., is so rich with interest and so widespread in the areas it intersects that researchers outside existing defense research (as well as those in it) should find it stimulating and an opportunity for creative invention.

The fact that normal matter versions of many critical relevant antimatter experiments exist implies that a very broad cross-section of the physics community has applicable experience which lends itself to concerted work, with the expectation then of relatively prompt resolution of certain crucial antimatter questions: namely, a reasonably confident perspective of basic feasibility issues in a 5-year period.

ANTIMATTER ISSUES — PROGRAMMATIC ASPECTS

A. Basic research, engineering questions

- Reasonable scale production at adequate efficiencies
- Compact long term storage, especially suitable for space vehicle use

B. Some applications, if basic research, engineering questions appropriately answered

Propulsion Uses	Power Generation	DEW Uses	Other Uses
— Very broad technologies	— Orbital prime power, for engagement role	— Light weight systems	— Classified additional special weapons roles
— Exhaust velocity range	— Simplest scheme has technology commonality with simplest propulsion scheme (use heated working fluid)	— Hard kills	— Generalized portable stored energy
● $10 < V_e < 100$ Km/sec $\left(\frac{T}{W}\right) \sim 1$		— Particle beam, or pumped lasers relying on very short duration energy release	— Product availability can raise still further uses
● $V_e \sim$ up to C $\left(\frac{T}{W}\right) \ll 1$		— Some attractive special/unique phenomenol-ogies	
— Opens many new mission possibilities			
— Replacement for current missions depends on product cost			

Notes:
- Implementation of applications differ in complexity
- Some systems uses can combine several types of applications
- Tolerable product cost is applications sensitive

Reasonable applications of antimatter and annihilation energies to various interesting uses outside the current use in very high energy physics generally presuppose the solution of basic production and storage problems (these will be further discussed later).

If these basic problems are resolvable, a very wide range of potential applications exists. Most of these applications can be generally discussed on an unclassified basis.

Of special interest are, e.g., propulsion applications. Using annihilation energies gives us means for accessing effective exhaust velocities from, say, 10 Km/sec to a major fraction of light velocity (of course the conceptual engine designs will be varied and will reflect the exhaust velocity ranges sought). Studies exist on various implementation schemes. Basically, the promise of antimatter can here be very simply illustrated by considering a "mix ratio" r = amount of normal matter/amount of antimatter and calculating the effective attained temperature of the mixture as ~ 2 GeV/r (so that, e.g., mixing one metric ton of normal hydrogen with one milligram of antihydrogen gives an upper mixture temperature of ~ 2 eV). Naturally, ensuring that this mixing produces high temperatures and that the energy does not largely escape from the mix is part of the art of utilizing annihilation energies.

These considerations and implementation strategies can be (and are being) gone through much more carefully. It is already clear, for example, that we can in principle perform propulsion missions which are otherwise "impossible" (because the customary exponentially increasing total mass/payload mass ratios are very dramatically reducible through use of antimatter). If, for example, we consider a very demanding mission for conventional propulsion systems requiring a velocity increment which is a large multiple of the exhaust velocity obtainable by conventional means, the exhaust velocity obtainable from annihilation energies in practical systems can be such that for the same mission the ratio velocity increment/exhaust velocity is substantially below unity.

FUNDAMENTAL CONCEPT DEFINITION ISSUES — SUMMARY

- Ground production, handling, interim storage of antimatter
  - Production is accelerator based (current technique)
  - Production level: milligrams per year interesting
  - Production scaling considerations roughly understood
  - Engineering scale-up difficult and complex

- Compact, transportable antimatter storage
  - Difficult; but many options to explore
  - Current research techniques applicable
  - Most of critical RDT&E can be done with normal matter
  - Success uncertain; but paths to resolve uncertainties definable

From currently known work on antiproton production and collection at the three major nuclear physics centers, we have some conception as to what would be involved in scaling up production related facilities. On a continuous basis, the rate (antimatter/unit time) scaleup objective might be a factor of $\sim 10^5 - 10^6$. Part of this scaleup would come from dedicated, more efficient production/collection, part from investing much more energy in the process. The required scaleup would be a massive and difficult engineering task.

Today at least two basic methods for storing antimatter have been demonstrated, at widely different levels. A number of other possibilities (with probably more eventual applications interest) appear promising in principle. Experiments seem required to resolve the key issues (that is, in cases where analytical proofs or disproofs of storage implementation are not practical, and where the environmental and interactive features of the physical situation are too complex to be amenable to confident analysis).

We repeatedly emphasize these two basic facts:

- There are no seemingly easy paths to use of annihilation energies, and many uncertainties of a basic and practical nature currently impede such use.
- Large scaleup factors/performance improvements are needed to make use practical, even if basic uncertainties are removable.

Why then consider use of annihilation energies at all? First, utility and payoffs could be singularly high. Second, effective removal of *basic* uncertainties (go, no go) is almost certainly possible in the near term--i.e., within 5-7 years. Third, arriving at a go or no go conclusion is an effort intrinsically interesting, productive, and attractive, likely to induce a resurgence in a great many physics and engineering disciplines.

VARIOUS ESTIMATES OF BASIC ANTIPROTON PRODUCTION

$\dfrac{\bar{P}}{P}$, antiprotons/ incident proton

$\left(\dfrac{1}{\sigma}\, E\, \dfrac{d^3\sigma}{dp^3} \right)$

Proton energy, GeV
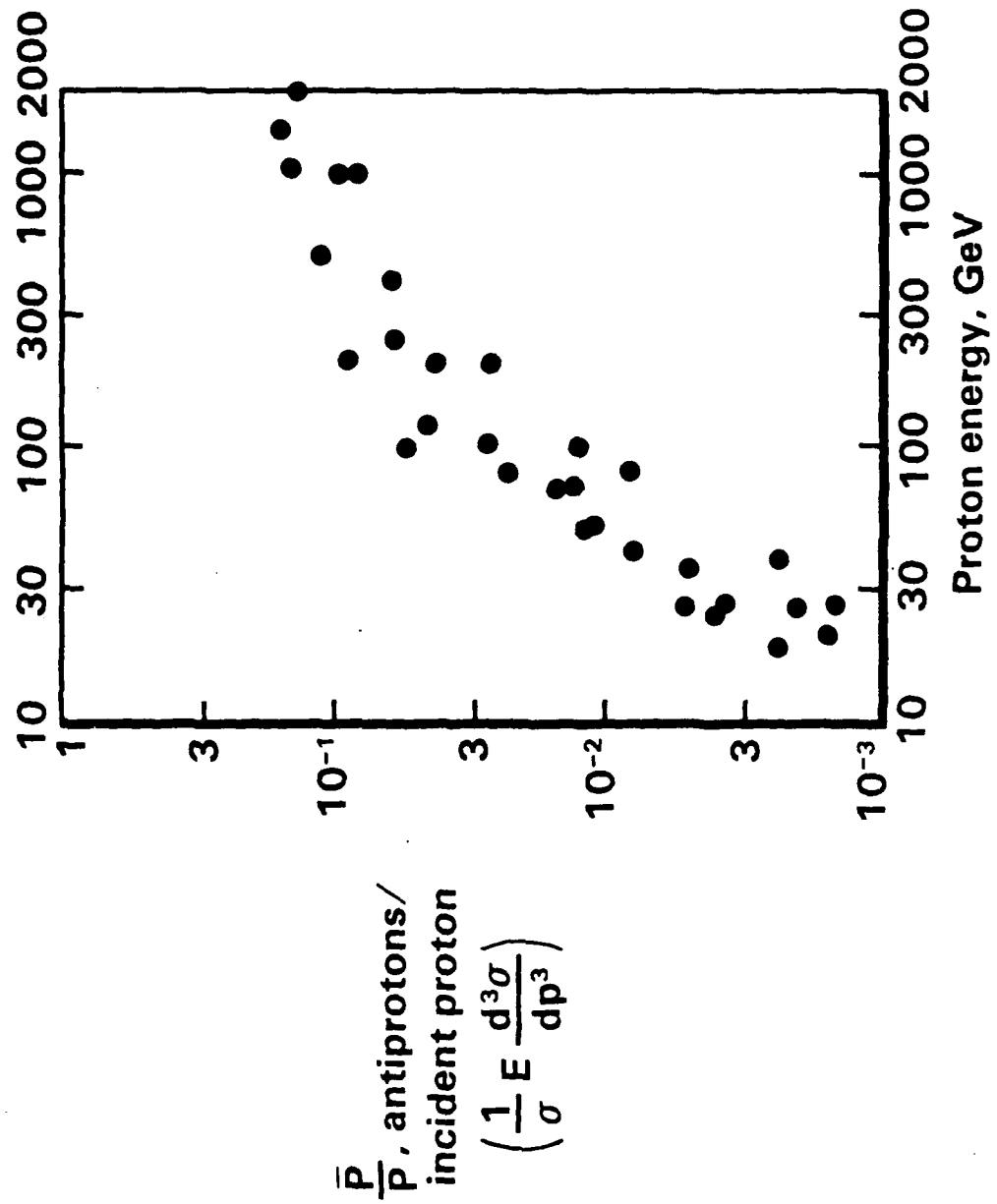
For production of antiprotons by interactions produced from the collision of a high energy proton beam on a metal target at rest in the laboratory system, the fundamental parameter of interest is the number of antiprotons produced per incident proton of energy E. While a simple relativistic calculation shows that the threshold kinetic energy E_T to produce n nucleon-antinucleon pairs, M the particle mass, is $E_T \sim 2$ Mn(n+2), so that n antinucleons could result, the actual number resulting is very much smaller because a great many competing reactions arise. The chart shows the actual antinucleon production in the face of the competing reactions. Thus, a 500 GeV proton, if all its energy could be devoted to producing appropriate antinucleons, could produce $\sim$ 15 antiprotons. Competing processes in today's techniques lower this number to $\sim 10^{-1}$, as shown.

The chart collects data from a number of sources. It shows that appreciable uncertainties in absolute values of production still exist, so that some mean values must be used. Even as late as the early 1980s production cross section corrections of a factor of $\sim$ 2 were needed for the CERN machines.

ANTIPROTON ($\bar{P}$) MANUFACTURING CONSIDERATIONS

- $\lambda = \dfrac{\text{Number } \bar{P} \text{ produced and collected}}{\text{Number } P \text{ in incident beam}}$

- Linearized, factored λ relation for discussion (more precise relations available, used for high λ estimates)

  $\lambda = \dfrac{b}{a} \cdot M \cdot dM \cdot S \cdot A \cdot B \cdot C$

  (given by nature)   (the engineering challenge)

  Where: a = PP inelastic cross section

      b = Invariant $\bar{P}$ production cross section $\Big\}$ P momentum dependent

      M = $\bar{P}$ momentum at production peak ($\sim 10^{-1}$ P momentum)

      dM = $\bar{P}$ momentum slice captured $\Big\}$ Large payoff factors

      S = Solid angle of collection system

      A = Absorption factor: P,$\bar{P}$ in target (~ 0.3 easily practical)

      B = Collection beam transport efficiency (~ 0.8 possible)

      C = Efficiency, all other losses (perhaps ~ 0.7 possible)

- Example: For Soviet 70 GeV IHEP machine, $\dfrac{b}{a} \sim 1.3 \times 10^{-2}$, M ~ 5.5 GeV/C.

  dM ~ 0.35 GeV/C, S $\sim 1.5 \times 10^{-2}$, $\lambda \sim 6 \times 10^{-5}$

- Basic energy efficiency: $\dfrac{\text{Number } \bar{P} \text{ produced per incident proton}}{\text{Energy of incident proton}}$

  - Relatively flat peak between 70 and 700 GeV P energy
  - Selection of P energy based on other design considerations

- Conclusions on achievable λ values

  - $\lambda \sim 10^{-4}$: can do now
  - $\lambda \sim 10^{-3}$: difficult near term goal
  - $\lambda \sim 10^{-2}$: not impossible longer term goal

Given the basic antinucleon production, one can now estimate the fundamental parameter λ, defined as the number of antiprotons produced and collected divided by the number of protons in the incident beam, employing a highly linearized formulation generally used in the high energy physics labs. λ accounts for collection as well as production considerations. Significant improvement in λ can come from two basic sources--operating at appropriate proton energies, and being able to collect over the broad exiting antinucleon momentum range along with an appropriately broad collector solid angle. Collector designs to accomplish this are naturally complex and difficult to engineer, as we try to collect more and more of the particles. λ values in the 10^{-3} to 10^{-2} range would likely require proton energies in excess of the largest currently being implemented ($\sim$ 120 GeV at Fermilab), while $\lambda \sim 10^{-3}$ might still be achievable at roughly such energies.

The basic energy inefficiencies in producing antiprotons from protons of energy E (in GeV) are now evident--the ratio of interest is:

$$\frac{\text{stored energy in antimatter}}{\text{energy to produce, collect antinucleon}} \sim \frac{2\lambda}{E}$$

and of course the process of imparting an energy E to a proton is itself not 100% efficient. Despite this, use of antimatter evidently makes sense in specific circumstances.

It should also be remembered that there is a very large absolute scale up issue at any value of λ, if we are to produce operationally significant amounts of antimatter.

There are theoretical possibilities for collector designs which may be promising and which differ from today's designs in significant ways.

BASIC POWER NEEDS FOR $\bar{P}$ PRODUCTION

- Power needs critically dependent on λ value achieved

- Basic power consumption: $\bar{P}$ beam power major driver

- Set nominal production level: 10 milligrams per year ($\sim 2 \times 10^{14}$ $\bar{P}$/sec.)

- For a range of λ values:

	$\lambda = 10^{-4}$	10^{-3}	10^{-2}
Proton beam flux (P/sec)	$\sim 2 \times 10^{18}$	2×10^{17}	2×10^{16}
Proton beam current, milliamps:	~ 320	32	3.2
Proton beam power, gigawatts: (select P energy $\sim$65 GeV)	20	2	0.2
Required input power (eff. $\sim 1/2$), gigawatts:	40	4	0.4

- Powering options:
  - External (note — gaseous diffusion plants used $\sim$ 6 gw external power)
  - Recover some energy by decelerating exiting P beam
  - Self power (use BNL electronuclear technology concepts)

In this chart we take the λ values just suggested and compute the basic power amounts invested to achieve a given amount of antinucleon production (in the example we use a production level of 10 milligrams/year). We assume a dedicated facility, continuously producing.

Some ways of recovering/saving the power needs shown, possibly up to the case where complete self-powering is achievable, can be suggested. These ways involve the energy production possible in multiplying materials such as uranium (see page 21). However, it is also pointed out that there are precedents for large facilities relying on large amounts of external power.

Power savings will generally require somehow using the proton beam which *exits* from the antiproton production target. That target will generally operate as a transmission target, so that the exiting proton beam is a sizeable fraction of the incoming proton beam. Serious attention to recovery/self-powering options requires more quantitative information on the spectral properties of the exiting proton beam to obtain adequate details on the problems of utilizing this exiting particle stream.

PRODUCTION PLANT EXAMPLE — POWER INTERNALLY GENERATED
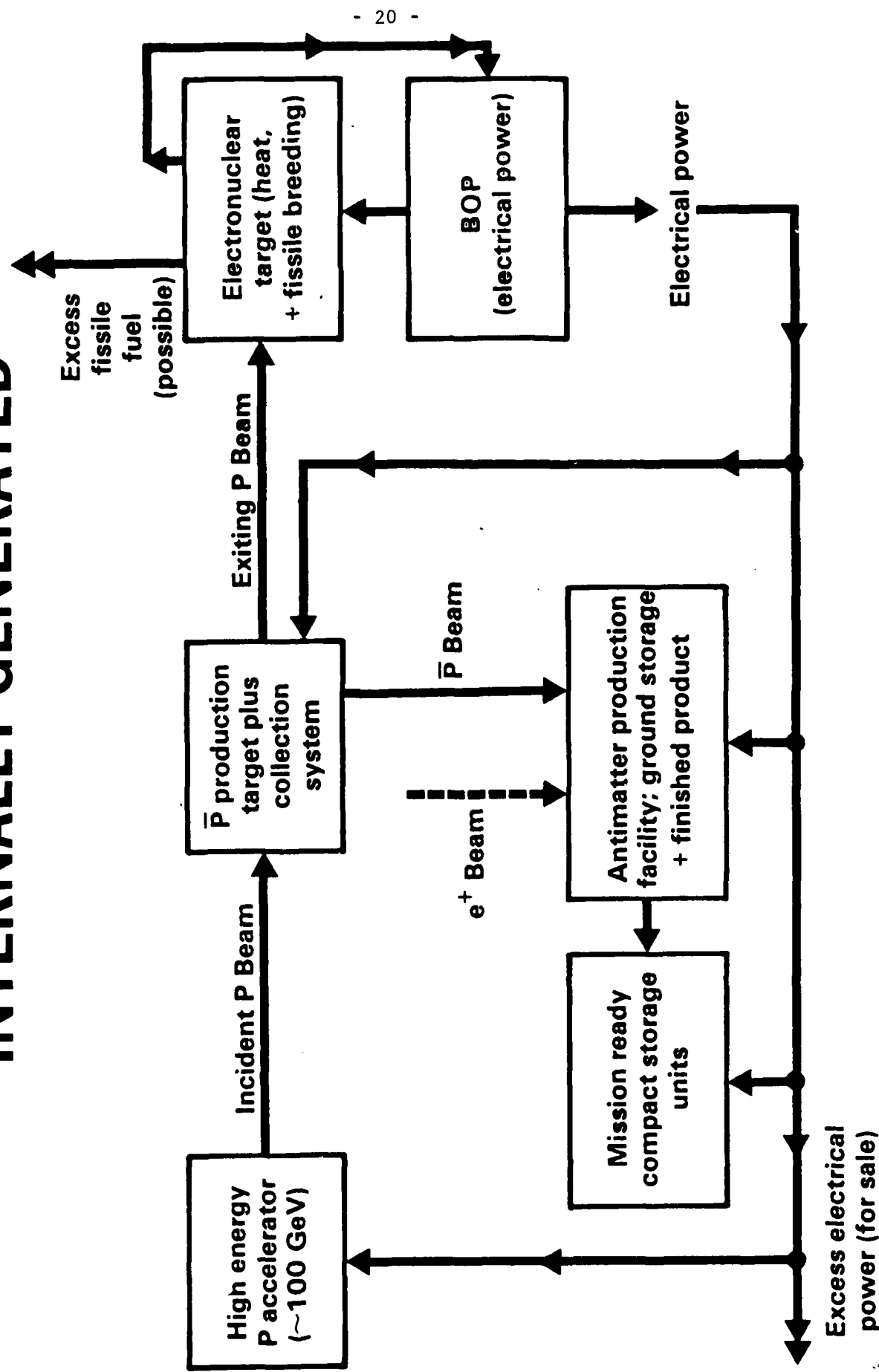
One scheme for self powering involves running an appropriately selected proton stream emerging from the antiproton production target into an electronuclear assembly of the kind previously considered extensively by the Brookhaven National Laboratory. This scheme uses evaporation/spallation reactions to produce neutrons in assemblies containing depleted, natural, or enriched uranium to produce heat and fissile material (production of both increases rapidly as the enrichment of the assembly increases). The heat is used to run a conventional electricity producing plant (enriched fuel beyond a certain fraction enrichment might also be sold).

Some forms of antimatter might be stored as antihydrogen atoms or molecules, requiring provision of positrons, facilities for enhanced recombination of positrons and antiprotons, etc. Provision of positrons can be done in several ways and is not as constraining as antiproton production.

This chart then shows a conceptual scheme for how a self-powered antimatter factory might be arranged. This possibility is probably most attractive when very large power investments are implied, as the subsequent chart illustrates. Such a self-powered factory poses many challenging problems of material balances, energy balances, process "self-consistency," and the like, which would be intriguing to evaluate.

It is tempting to characterize antimatter production as a "by-product" of an electronuclear plant producing electricity and fissile fuel; however, the needed proton energies are much lower for such purposes than the ~ 100 GeV protons attractive for antimatter production. The combined plant as described is in any case an interesting self-standing symbiotic plant concept, whose design is susceptible to a number of variations.

P̄ PRODUCTION COST ESTIMATES

- "Cost" depends on economic philosophy, resultant assumptions

- Simplifying assumptions used:
  - Basic accelerator system: 0.3B + $500/Kw beam power (very large systems)
  - Production factory: 0.3B + $200/Kw beam power (very large systems)
  - Electronuclear (EN) systems: 0.4B + $600/Kw beam power (very large systems)
  - External power: 4 cents/Kw hour ($320 million/gw year)
  - Capital cost amortization: 40 years
  - Operating costs subsidized: i.e., 0 costs
  - 10 milligrams/yr. production: 400 mg in amortization period

- Cost elements

	$\lambda = 10^{-4}$	10^{-3}	10^{-2}
- Capital cost (10^9)			
With EN system	27	3.6	1.26
Without EN system	14.6	2.0	0.74
- Electricity costs (without EN system)	12.8/year	1.28/year	0.128/year

- Costs, per milligram per year

- Without EN system	$1.3×10^9	$133×10^6	$15×10^6
- With EN system	68×10^6	9×10^6	3×10^6

- Note on costs
  - Some applications could easily allow production costs of several × 10^8/mg
  - Adequately credible cost estimates warrant much deeper fundamental systems studies
  - Interesting cost range relatively robust to capital cost variations

This chart shows simplified estimates of antimatter cost. Cost components are generally normalized to the proton accelerator beam power. Resultant costs are shown for both reliance on external power and reliance on self-powering.

The cost estimates enclosed by the dashed line appear sufficiently constrained to be *a priori* acceptable for many potential applications. These costs are of course uncertain, since adequately comprehensive estimates have not yet been made. On the other hand, these costs could change by large factors and still be tolerable for the mission applications contemplated.

The production costs cited *are to include costs* of some steps beyond just antiproton production (page 20). We may wish to produce atomic or molecular antihydrogen in a condensed phase, for example, as the standard product. There are in principle several ways one could go from the antiproton beam step to, say, the molecular antihydrogen step, differing in where the positron is introduced and what stage of antihydrogen formation one emphasizes. Starting from an antiproton beam, one might wish to trap antiprotons, then form and continue to trap successively atomic and molecular antihydrogen; or, starting from an antiproton beam, one might attempt to form "on the fly" an atomic antihydrogen beam, then a molecular antihydrogen beam, and then finally trap, store and condense the molecular beam, etc. This shifts ever present difficulties (for example, the energy release in the atomic to molecular conversion) to different stages of the total production process, and will require more detailed cost estimation once some (if any) particular process path is decided to be realizable.

HAVING PRODUCED $\bar{P}$ — THEN WHAT?

- **Deceleration, cooling stages on way to compact storage**

  – Phase space control, matching (pre-storage to final storage)

  – Generally want very low velocity/$\sim$ stationary particles for insertion into compact storage

  – Transition issues ($\bar{P}$ production $\longrightarrow$ compact storage) non trivial

- **General issues for compact storage techniques**

  – Characterize storage parameters; special cryogenic/vacuum requirements

  – Verify storage lifetimes

  – Evaluate handling issues; insertion/extraction rates, losses

  – Verify storage design principles

  – Estimate storage volume, mass, power implications

  – Role of portable $\bar{P}$ traps for early experimental purposes (mini-facilities)

- **Interesting range of stored material amounts (initial goal)**

  – $\sim$100 μg — $\sim$10 mg, per compact storage unit

  – Many operational applications possible with such amounts

Our problems have only begun after we have initially produced and collected the antiprotons from the production target. Control of the antiparticle phase space volume is critical. Typically, the currently produced antiprotons initially fill a phase space volume of ~ 10^2 eV-s at a mean energy of perhaps ~ 3-12 GeV (dependent on the incident proton energy). A compact storage unit (a "trap") may have a phase space volume perhaps 10^8 or more down from this. Thus several intermediate stages of deceleration and cooling will be required for matching, and the phase space density must be very carefully controlled. Current plans for antiproton trapping at very low energies propose use of the LEAR ring at CERN, followed by further phase space volume adjustments. (Current proposals to utilize LEAR in this way stem from an Italian team, a University of Washington team, and more recently a LANL team).

Some of the objective measurement issues and considerations for useful compact storage are shown on the chart.

The compact storage units which would be suitable for a wide range of applications are required to store minuscule amounts of material, by conventional standards. To put these amounts in context, remember that these tiny amounts still reflect the following available energy content:

    100 μg --- 4.4 metric tons TNT equivalent

      1 mg --- 44 metric tons TNT equivalent

     10 mg --- 440 metric tons TNT equivalent

The challenge of storage lies in simultaneous satisfaction of several requirements. Requirements include useful product forms and amounts; compact, lightweight storage; long storage lifetimes; manipulation with acceptable losses; and tolerance to specified perturbations (acceleration levels, etc.). For example, we can today build traps storing antiprotons; *current techniques and designs* for these can prove useful in all requirements save the first (amounts), where we want another factor of perhaps 10^6-10^7.

SOME POSSIBLE OPTIONS FOR COMPACT STORAGE (NON-EXHAUSTIVE LIST)

- Storage in static or dynamic electromagnetic traps (e.g., Penning traps)

  - Used in past to store antimatter
  - Operational suitability limited (unless closer circumvention of space charge)
  - Transportable version useful to conduct $\bar{P}$ experiments

- Storage in special sites in normal matter

  - E.g., in liquid He bubbles

- Storage of spin polarized antihydrogen

- Storage of condensed state antihydrogen

  - Eases space charge issues
  - Stored in levitated state in cryogenic enclosure

- No certain route to compact storage yet — But:

  - Significantly large number of possibilities
  - Need initial survey, assessment, critical experiment formulation
  - Much current research relevant (e.g., Univ. of Wash.)

Compact storage suitable for storing antimatter for operational applications is an area crying out for conceptual invention. Some possible approaches are shown on the chart.

The standard electromagnetic traps are not necessarily restricted to the conventional types only.[1] Portable traps are considered in the experiment phase because, while they need "filling" at an accessible antiproton production facility, they could in principle subsequently be moved to other sites where more convenient experimentation might be possible and where more broadly based experiment teams could engage in "hands-on" work with antimatter.

The University of Washington team is specifically mentioned because of an extensive history of positron trapping and experimentation (for CPT experiments) and a carefully formulated plan to enlarge this work to antiprotons,[2] again for CPT related work (e.g., g-factors, inertial and gravity mass comparisons). Experiments using Penning traps range all the way from storing essentially single antiparticles for precision measurements (CPT experiments) to experiments on "maximum" filling with antiparticles (using current designs, sizes, and fields, perhaps 10^{10}-10^{12} particles/cc may be achievable).

Even a casual survey of the compact storage problems suggests that research spanning an enormous number of physics and engineering subfields might be usefully involved. Many trapping and storage issues can be tackled initially using normal matter, and then involve a great many disciplines of classical atomic physics. It should be noted that schemes other than the ones indicated, and additional variants, have been suggested for compact storage.

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[1] See, for example, the collection of reprints on interesting related techniques designed for long-term storage of electrons, *Reprints on Pure Electron Plasmas*, Univ. of California, San Diego, Dept. of Physics, October 1984.

[2] See e.g., Kells, Gabrielse, Heimerson, *On Achieving Cold Antiprotons in a Penning Trap*, IX Conf. on Atomic Physics, July 23-27, 1984, and also Kells, *Remote Antiproton Sources*, to appear.

A BRIEF SAMPLE OF SOME BASIC RESEARCH, EXPERIMENT PROGRAMS

- **Compact Storage-Related**

  Case: Normal matter experiments
  - Finished product
  - Forming the finished product

  Production-Related
  - Basic investigations
  - Target design
  - Collection System design
  - Intermediate stages
  - Factory conceptual designs
  - Powering issues
  - Total Plant Cost Estimates

- **Performers for such programs:**
  - Most can be done in academic, industrial laboratory settings
  - <u>Some</u> would be best done at National Labs
  - Most critical experiment programs not costly ($ few hundred K to few million)
  - Most issues relatable to known experimenter/experiment teams

- **What such basic research, experiment programs would achieve**
  - Formulate, do critical studies and experiments
  - Perform early demonstrations
  - Very substantial upgrading of assessment of antimatter promise
  - If promise continues, give design inputs to proto type phase

It is possible to define and formulate perhaps several hundred critical studies and experiments relevant to antimatter production and compact storage. The chart introduces a sample of those critical studies and experiments.

The issues involved are broad; the majority can be considered by teams outside the FNAL/CERN milieu, so that the experimental team base encompasses much more than personnel at the major high energy physics centers.

Rand has considered to some substantial extent where suitable researchers and research teams could be found, matching identified critical studies and experiment needs. Rapid experimental progress on problems relevant to antimatter appears possible because so many of these problems are first amenable to prototypical normal matter experiments which do not depart too far from programs already in active research.

We emphasize again the opportunities for fundamental investigations of antimatter. For example, if one has produced and trapped antihydrogen, measurements of the Lamb shift in antimatter can be done, involving a meticulous coupling of old and new techniques.

All in all, there is a great deal of elegant physics and engineering to be tapped, not only in fundamental investigations involving antimatter, but also, and at least as compelling, in the great many normal matter experiments with which one can start off. For example:

Develop tunable Lyman-α sources (4 wave mixing in Mercury).

Pi-pulse cooling schemes.

$\bar{H}$ to $\bar{H}_2$ - laser stimulated recombination; use of polarization phenomena.

Spin-polarized $\bar{H}$ - non-contacting traps, limiting 3-body recombination.

Many paths for condensed $\bar{H}_2$ - trapping/formation alternative options.

SOME EXAMPLES — PRODUCTION-RELATED RDT&E

- Basic investigations
    - Best estimates of production CS ($\bar{P}$; e^+)
    - Best estimates of $\bar{P}$ spectrum
    - General formulation of $\bar{P}$ production + collection problem (non-linearized form; utility of approximations)
    - Collection yields vs. basic parameters (finite target; acceptance; matching conditions; etc.)
    - Fraction of basic $\frac{\bar{P}}{P}$ ratio achievable
- Target designs
    - Methods for alleviating high target heating, stresses (inc. bunching, debunching issues)
    - Relative promise of pulsed vs. continuous operation; proton beam dither over target; target motion; etc.
    - Tolerable P beam intensities
    - In-target focusing
- Collection system design
    - Solid angle
    - Momentum bites
    - Lens designs; depth of focus, target absorption; etc.
    - Absolute costs or relative costs vs. phase space volume
    - Total beam transport system design
    - Matching to storage rings/deceleration stages (cooling requirements)
    - Unconventional collector possibilities

SOME EXAMPLES — PRODUCTION-RELATED RDT&E (Cont.)

- Intermediate stage considerations (i.e., between $\bar{P}$ production and production of final product inserted into compact storage)
    - Cooling stages needed
    - Deceleration stages
    - For antihydrogen production, optimizing conditions for $\tilde{P}$, e^+ recombination (process path)
    - Energy balances, material balances
    - Achievability of matching; self-consistency of total processes
    - Power needs; optimization of energy efficiencies
- Factory conceptual designs (factory = complete cycle, including P beam and going to product to be stored in compact storage)
    - Overall system balances
    - Process loss estimates
    - Estimate of power needs
    - Survey of powering options (cost, complexity, schedule issues)
- Power production issues, if self-powered by electronuclear facilities (BNL design)
    - Large target measurements in U assemblies
    - P spectrum emerging from $\bar{P}$ target
    - Conceptual design for power production using emerging P
    - Power cost best estimates
- Total plant cost estimates
    - Accelerator, production factory, electronuclear facilities, etc.
    - Siting considerations
    - Applicable cost element scaling laws
    - Implications for product cost ($/mg)

The following two charts list some of the RDT&E study and
experiment issues associated with production of antimatter and the
associated product cost. Many of these are already partially treated in
the appended references, but each item listed warrants much further
consideration.

For example, the CERN target has a nominal energy deposition limit
of ~ 185-200 J/gm, at which point the temperature rise is greater than ~
10^3 C and shock waves begin to form which can fracture the target. This
limit corresponds to a beam of ~ 2×10^{13} $P/_{mm}2$; still higher depositions
will cause target material depletion and reduction of antiproton
production. Even for a $\lambda = 10^{-3}$, a proton beam flux of ~ 2×10^{17} P/sec
is needed to achieve a production level of 10 milligrams of antimatter
per year. Many solutions can be proposed (and some have been
considered) to heating, stress, etc. problems caused by such intense
beams. One possibility, for example, is to form a number of beamlets
and at the same time move each beamlet relative to its associated target
at rates significantly greater than the target shock velocity of ~ 0.3
mm/μsec. In turn, one option for this relative motion is to move the
target against a fixed proton beam (to circumvent the problems of having
the antiproton collection system track the motion of the proton beam in
the case where the target is fixed). The design challenge is then to
move targets at velocities of perhaps ~ 1.0 mm/μsec relative to the
fixed beam, continuously. Conceptual schemes for this exist.

In a similar way, each item in the following two charts can be
enlarged upon. The important point to emphasize, then, is that well-
defined approaches can be formulated to resolve the uncertainties which
currently abound in the RDT&E issues posed by antimatter production and
collection.

For high energy physics, needs to reaccelerate accumulated antiprotons
to very great energies focuses attention on high quality beams. Maximizing
antiproton production may modify or give different emphasis to process
bounds.

SOME EXAMPLES — REPRESENTATIVE NORMAL MATTER STORAGE-RELATED EXPERIMENT

FINISHED PRODUCT EXPERIMENTS

- Form condensed phase hydrogen
  - Test nucleation phenomenology, etc.
- Form in two states
  - Exactly neutral
  - Some charge excess
  - Measure properties of condensed state
- Levitate in condensed state (diamagnetic)
- Levitate in condensed state
  - DC electrostatic servo
  - G-force tolerances
- Lifetime of condensed levitated state
  - 1 to 10 mg amounts
  - Vapor pressure, specific heats, vs. enclosure temperature
  - Vacuum level
- Removing constituents from condensed state
  - Removal process options
  - Nature of constituents
  - Nature of remnant
- Removal from levitated condensed state
  - Removal should be non-destructive of levitated mass
  - Nature of constituents, remnants
  - Controllability of materials
  - Losses, efficiencies vs. removal process
- Storage of spin-polarized atomic H
  - Leakage, 3-body recombination
  - Storage densities, lifetimes

SOME EXAMPLES — REPRESENTATIVE NORMAL MATTER STORAGE-RELATED EXPERIMENT (Cont.)

PROCESS EXPERIMENTS (FORM FINISHED PRODUCT)

- Enhanced H_2 formation: controlled recombination atomic to molecular para state
- Laser, variable magnetic field cooling of H atoms, molecules
- Slowing, trapping, cooling (atoms, molecules)
  - Include crossed laser resonance radiation schemes
- P, e^- formation of H; enhancement schemes
  - Optimizing conditions for recombination, etc.
- Establish input parameters for final slowing, trapping, cooling
  - Set required production process exit conditions
- Establish nucleation conditions
  - Starting from hydrogen gas
- Attempt partial/complete process runs without contacting material walls
  - Successive stages — P, e^- formation of hydrogen
    - Control, slow down, cool hydrogen beam
    - Trap, further cooling
    - Dense, cold gas
    - Produce aggregated state (nucleation, etc.)
    - Verify properties of aggregated state
    - Transfer to compact storage
    - Levitate
    - Measure lifetimes, losses, efficiencies
    - Contolled removal of material from levitated state
    - Manipulation of material (removed material, remnants)
    - Gauge transferability of experiments to $\bar{H}$, $\bar{H}_2$ case
- Define necessary antihydrogen experiments
- Etc.

The next two charts discuss just a sample of some compact storage normal matter experiments. These experiments consider two classes of initial conditions.

- Finished product experiments: prepare a condensed phase of hydrogen, e.g., a hydrogen solid, in any convenient fashion and then extract some of this solid and introduce it by conventional manipulation techniques into a cryogenic enclosure where it is levitated in one of several basic ways. In its levitated state certain measurements are performed, and various ways of controlled manipulation of pieces of the solid are experimented with.

- Forming a condensed hydrogen phase ab initio (i.e., from protons and electrons) without contacting material walls and with noncontacting manipulation means (thus giving a prototype scheme for forming and manipulating antihydrogen).[3] These classes of experiments would normally be much too complicated to contemplate unless one had in mind the ultimate extension to antihydrogen. Again, each step of this attempted process can be defined in more detail. E.g, there are several occasions where heat must be removed. Possible implementations of controlled heat removal have been proposed, but clearly need experimental trials.

The main point to emphasize again is that it seems possible to define, plan, and conduct experiments to remove or alleviate uncertainties in the practicality of implementing suitable storage schemes. In the process of performing such experiments, research paths of interest to basic research groups are almost limitless.

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[3]There have very recently been performed some relevant experiments: "Stopping Atoms with Laser Light," Prodan et al., *Phys. Rev. Lett.* 54, 992, 11 March 1985; and "Laser Manipulation of Atomic Beam Velocities. Demonstration of Stopped Atoms and Velocity Reversal," Ertmer et al., *Phys. Rev. Lett.* 54, 996, 11 March 1985. See also "Magnetic Trapping and Cooling of Atomic Hydrogen," Hess, to appear.

ANTIMATTER PROPULSION SYSTEMS

Antimatter Isp ranges
(I_{SP} = specific impulse = exhaust velocity/g)

Thrust/Weight $\ll 1.0$;
Conceptual designs exist

Thrust/Weight ≥ 1.0;
Conceptual designs exist

~3×10⁷ 10⁷ 10⁶ 10⁵ 10⁴ 3×10³ 10³

Propulsion system comparisons: Required single stage mass ratios for representative high thrust missions

Note: V_m = mission characteristic velocity

$$\text{Mass ratio} = \frac{\text{payload + inert + ejecta}}{\text{payload + inert}}$$

Tactical/operational mission
(Double reverse orbit from space station:
V_m = 31 km/sec)

storable propellants
hydrogen-oxygen
nuclear thermal
antimatter

Specific point design:
I_{SP} ~ 2600

Scientific/operational mission
Very fast Earth/Mars transit:
V_m = 40 km/sec

storable propellants
hydrogen-oxygen
nuclear thermal
antimatter

10⁶ 10⁵ 10⁴ 10³ 10² 10 3 1
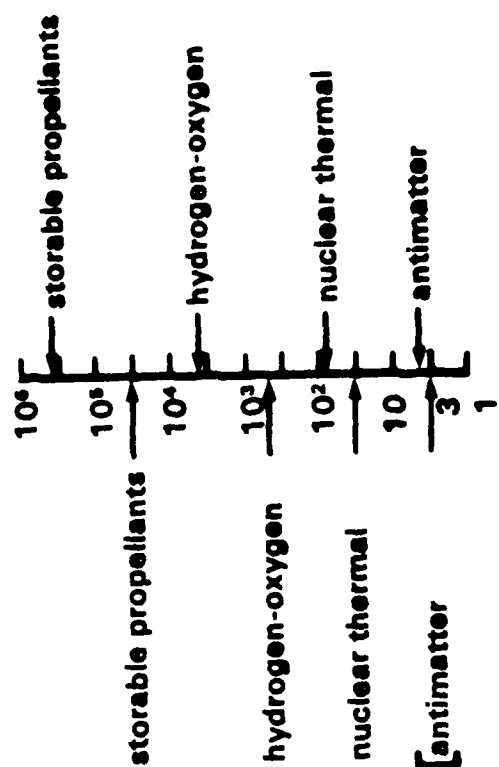
Some illustrations are now discussed to put antimatter usage into perspective. The illustrations consider propulsion applications earlier alluded to on page 11, as the chart shows.[*]

Because of the very high energy density of antimatter, a simple optimization analysis can be made which minimizes the amount of antimatter needed as a function of the mission characteristic velocity, V_m. We find that the minimum antimatter consumption arises when we choose the exhaust velocity, V_e, to be ~ 0.63 V_m; and that minimum consumption is then ~ a $M_e V_m^2/C^2$, where a is a constant we can take usually roughly between 1/2 and 1, M_e is the vehicle empty mass (M_e = payload mass + inert mass), and C = light velocity. To take a specific example from the chart, if we wish to have M_e = 1 metric ton for the double reverse orbit mission from a space station (a vehicle leaves the station to contact a counter-orbiting vehicle, and returns), the antimatter consumption is of the order of 5-10 mg. The optimization analysis in effect assumes that the exhaust velocity V_e is "tailored" to fit the necessary V_m. This I_{sp} tailoring is possible, to a certain extent, using available conceptual engine designs, and the analysis then holds reasonably well up to V_m ~ 1/2 C. If we fix I_{sp} then operation off this optimization can result in somewhat more than necessary antimatter consumption, but lower mass ratios than the "optimal" value of ~ 4.9 $\left(= e^{V_m/V_e} \right)$. The chart shows a case of this.

Possible engine design types are visualizable over an enormous range of I_{sp}. The relative merits of using antimatter of course increase dramatically when more demanding missions than those shown on the chart are considered. Many engine designs and other potential applications rest on good understanding of annihilation phenomenology, an example of which occurs in the next few pages. Many interesting theoretical and computational issues arise in further developing this understanding.

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[*]Further details are found, for example, in: *Antiproton Annihilation Propulsion*, R. Forward, AFRPL-TR-85-034, 15 August 1985; and *Some Examples of Propulsion Applications Using Antimatter*, B. Augenstein, The Rand Corporation, P-7113, July 1985.

TWO BASIC P̄ LETHALITY CASES

● Case 1 — *Ignore* pion absorption within annihilating nucleus
  - All produced pions escape annihilating nucleus
  - Energy deposition occurs in material thicknesses of tens of cms

● Case 2 — *Include* pion absorption within annihilating nucleus
  - Effect increases with atomic number
  - For heavy metals (e.g., U) more than ½ annihilation energy absorbed
  - Absorption ⟶ large local energy deposition, great many secondary particles
  - Calculations, some experiments exist (including CERN, 1983-84)

● *Both* cases simultaneously present in actual situation

● Importance of case 2 warrants consideration of:
  - Extended, improved code runs including secondaries
  - Special target mockups in pion and P̄ beams
  - Greatly expanded range of technical, system options

The immediate product of nucleon-antinucleon annihilations is almost wholly pions. The details of the subsequent reaction trains and the ultimate forms of the end products, their spectral attributes, the decay or capture mechanisms, etc., are discussed in our earlier documentation.

During the pion lifetimes one can treat the pions in two alternative ways. One way focuses on the pions which emerge from the nucleus in which annihilation has occurred. These pions are very penetrating particles which can go through materials many centimeters thick before capture or decay. Focusing on this way is particularly pertinent when one wishes to emphasize, for example, problems of target shielding against an upper limit of the flux of annihilation products.

Another way focuses on the subsequent history of the annihilation pions within the nucleus where annihilation takes place. The mechanisms here are believed to be relatively well understood, and are referenced in the appended bibliography. Some of the pions are captured within that same nucleus; some, while still escaping, are degraded in energy by scattering within that nucleus. These processes excite the nucleus, which then deexcites by particle emission, etc. Two effects arise: an initial intense local deposition of energy in the regions where annihilation occurs, and a shorter penetration length for a portion of the emerging pions. The effect is to increase the energy deposited per centimeter of travel within the target by the particles. Focusing on this way is more pertinent when, e.g., one asks for the maximum of the energy deposition in a particular region of the target.

The importance of intranuclear effects has been known for some time; estimates of the magnitude of the effects occur in some of the earliest comprehensive annihilation phenomenology papers (e.g., Agnew et al. in 1960).

COMPARING ANTIMATTER, MATTER BEAM LETHALITY

● Comparisons difficult to make commensurate

● Simplified energy deposition phenomena

$\overline{H}$ Beam

Three primary deposition sources:
1. Primary beam slowdown
   - Deposits beam energy $E_{\overline{P}}$ in range $\ell_{\overline{P}}$

Annihilation effects:

2. Intranuclear absorption, deexcitation
   - Deposits E_N in total range ℓ_N
3. Exiting pions
   - Deposits E_π in total range ℓ_π

H Beam

One primary deposition source:
   - Primary beams slowdown
   - Deposits beam energy E_P in range ℓ_P
   - For $E_{\overline{P}} = E_P$, $\ell_{\overline{P}} \sim \ell_P$

● Resultant simplified 1-dimensional overlapping deposition representation

$\overline{H}$ Beam

| $E_{\overline{P}} + E_N + E_\pi$ | $E_N + E_\pi$ | E_π |

$\ell_{\overline{P}}$
ℓ_N
ℓ_π

H Beam

| E_P |

ℓ_P
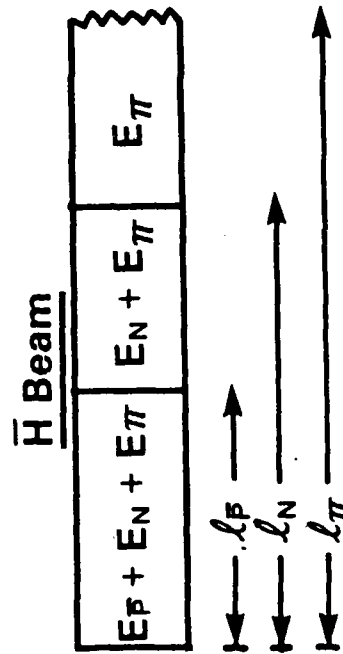
● Pion, proton ranges in matter
   - Annihilation at *arbitrarily low* $E_{\overline{P}}$ produces long range pions
   - Protons require $E_P \geq$ several hundred MeV for $\ell_P \sim \ell_\pi$

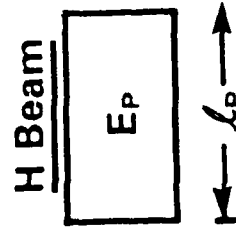Taking into account these intranuclear effects makes difficult comparative estimates of normal matter and antimatter beam target effects. A very simplified one-dimensional estimate indicates that the region where intranuclear effects are important includes, but is somewhat larger than, the region where annihilations occur (this latter region being also the region where the primary beam is slowed down and brought to rest). The reason for this is the smearing-out produced by the deexcitation phenomenology.

To the best of our knowledge, no fully adequate treatment of the combined phenomena involved in slowdown, intranuclear absorption, deexcitation exists; a really useful treatment would have to combine intranuclear absorption codes with transport codes, etc.

In the absence of such treatments, highly simplified estimates of the phenomenology are all that is available. The next chart shows some estimates of this sort.

Another aspect of annihilation phenomenology which differs significantly from normal matter beam phenomenology is that the kinetic energy of the normal particle is all important in producing target effects, while for antimatter beams the annihilation energy generally dominates the target effects at accelerator potentials convenient to work with. One could in principle work at much lower accelerator potentials for antimatter beams, although technical difficulties of beam formation, control, and shaping arise.

COMPARISON OF BEAM EFFECTIVENESS
(IN ALUMINUM)
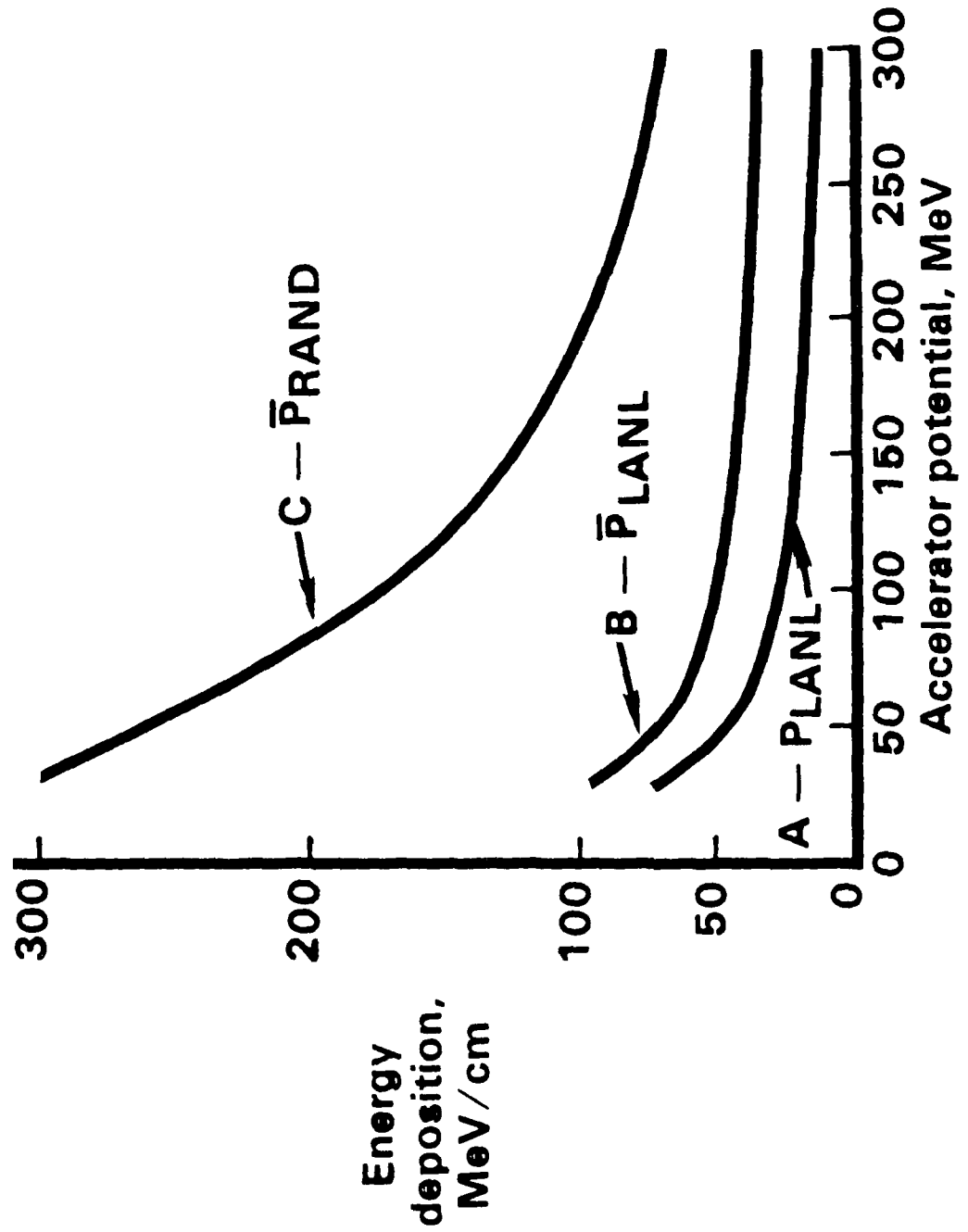
An internal memorandum from LANL[5] has presented a comparison of matter and antimatter beams based on a simplified model which includes energy deposition estimates for primary beam slowdown and, for the antimatter beam, the pion contribution--assuming all annihilation pions escape the annihilating nucleus. A suitably recast version of these estimates is shown on the chart (P_{LANL}, $\bar{P}_{LANL}$). The estimates do not consider any intranuclear effects.

We have attempted estimates on an effectively comparable simplified basis of the intranuclear effects (for aluminum; these effects increase relatively for heavier materials). The net result is shown on the chart ($\bar{P}_{RAND}$). The energy deposition cited is appropriate to the region marked $\ell_{\bar{p}}$ on the previous chart. In some cases the deexcitation phenomena can be of special interest.

This estimated effect of intranuclear phenomena is significant and important as reflecting more closely the actual physics of the target interaction. The estimates are uniformly approximations which in our view warrant improved calculations.

Those readers who wish a convenient and careful introduction to the physics and computational aspects of intranuclear absorption are advised to read first the leading paper in our Bibliography, by Clover et al. at LANL.

The chart shows the averaged energy deposition per particle resulting from the very simple model used. From this the particle flux required to obtain a given level of macroscopic energy deposition (e.g., in joules/gram) is calculable. The model can be improved in realism somewhat by using the actual form of the energy deposition, which e.g., gives the usual pronounced increase in deposited energy near the end-of-path. This Bragg effect can typically increase the average energy deposition in the last 1/10 of the path to about 3 times the overall average deposition, and becomes relatively more important to consider when one seeks very large energy depositions.

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[5]By O. Judd, April 24, 1984, *Antimatter Beams for Directed Energy Weapons.*

EXAMPLE DEVELOPMENT PROGRAM FOR ANTIMATTER PRODUCTION

- Three Phases — 20 year program

- Phase A
  - Basic RDT&E, conceptual designs on production, compact storage issues (YRS. 0-5)
  - Content along lines indicated previously
  - Go-no go decisions towards end of Phase A
  - Cost: $75-125 million

- Phase B
  - Prototype "mini-factory"
  - Demonstrate production, storage at acceptable parameter ranges
  - Two overlapping development efforts:
    - P accelerator + adequate targets (YRS. 0-6)
    - Rest of factory system (YRS. 4-8)
  - Accelerator to be useful even if no production finally sought (LAMPF II parameters or better):
    - Minimum beam current ~0.1-1.0 milliamps, minimum P energy 30-70 GeV
    - Emphasize intense secondary, tertiary beams (muons, neutrinos, kaons, hypernuclei, $\overline{P}$, etc.)
  - $\overline{P}$ production rate goal: 10-100 μg/yr (~2×10^{11} – 2×10^{12} $\overline{P}$/sec), or better
  - Costs: $500-1000 million

- Phase C
  - Full production factory goal: ~10 mg/yr. or more (YRS. 7-20)
  - Initiate if Phase B validates design concepts
  - Cost: ~$5-15 billion (depends on λ achieved; production levels)

- During Each Phase
  - Applications concept formulation, design refinement

If any applications of antimatter are to come to fruition, it will be necessary to resolve basic production and storage issues, and to test and develop appropriate production and storage technologies. One way of tackling these problems is by a 3-phase program of the kind suggested on the chart.

A critical part of Phase A will be discussed shortly (see page 52). The proposed Phase B contains one special feature--it treats a critical element in a "fail-safe" mode. Namely, we wish to start, concurrent with initiation of Phase A, a prototype accelerator capable of producing interesting amounts--much higher in rate ($\bar{P}$/sec) than is today available, by a factor of perhaps 10^2 to 10^3--of antiprotons. The required accelerator would have many compelling uses in medium high energy nuclear physics research. Such uses have been detailed in LAMPF II proposals, and reflect the very interesting physics resulting from secondary and tertiary beams producible only from fixed target systems. The proposed accelerator is a not too large upgrade of LAMPF II parameters. This strategy would result in an enduring dedicated physics tool, even if the outcome of Phase A were to be pessimistic about an antimatter "mini-factory."

At the same time, if the whole "mini-factory" were to be successful, a much more confident scaleup by another factor of 10^2 to 10^3 to an operationally sized production system would be possible (Phase C).

Phase A is generally to consider issues *approachable with existing levels of antiproton technology*, with emphasis on normal matter experimental precursors to critical experiments. We would largely focus on problems compatible with handling antiproton production rates $\leq \sim 10^5$/sec, accumulated levels $\leq \sim 10^{13}$, sizing, with a few exceptions, the normal matter experiments accordingly. Other than perhaps introducing portable/movable traps to make hands-on antiproton research more convenient, specific new antiproton production/accumulation facilities would not be mandatory (although desirable--e.g., a LEAR equivalent or other deceleration/cooling facilities at FNAL).

EXAMPLE DEVELOPMENT PROGRAM
BENCHMARKS/DECISION POINTS

Year	Activity
Phase A	
1	Survey, scoping, experiment + team choices; some experiments (first go - no go decisions)
3-5	Conduct of critical experiments; design point information (second go - no go decisions)
Phase B	
1	Accelerator design
4-5	First Beam (IOC as medium high energy physics facility)
4	Factory System design/construction initiated (if go from Phase A)
6-7	First Product (prototype factory IOC)
8	Design Product level (design validation — full factory go - no go)
Phase C	
7	Full Factory design initiated
8	Construction go-ahead (if go from Phase B)
15	First Beams
17	First Product delivery (full factory IOC)
18-19	Full scale operation
20	At least 10-20 mg. final product

Note: — Higher risk program eliminates Phase B
 — Pushes Phase C up by ~ 4 years (~ 16 year program total)

This chart summarizes the critical times implicit in the 3 phase development program suggested as an example. The first major go-no go decisions are to occur by end of year 1.

The schedule shown is felt to be "conservatively realistic" in a success oriented program--i.e., one in which each "go - no go" decision point happens to produce a "go." The mini-factory in Phase B is assumed to be designed to use external electrical power for its relatively modest power demands. For Phase C one would presumably have the option to select either externally powered or self-powered designs, with siting considerations, achieved λ values, and the goal for product yield being important factors in the selection.

The 5-year period for Phase A will strike some as optimistic, since it is often suggested that fundamental insights into the basic feasibility of applications-oriented antimatter technology will take a number of decades.

We believe that attitude is probably wrong. A great deal of relevant work in Phase A problem areas will involve use of normal matter. If the experiments with normal matter have satisfactory outcomes, then one should almost always have reasonably confident expectations that work with actual antimatter has a very good chance of success--albeit that work will entail additional complications, require added experimental subtlety, and demand more attention to handling, safety, and reliable operability issues. The successful normal matter work may be regarded as a necessary but not totally sufficient condition for confidence in being able to develop antimatter technology. At the same time, if there are basic stumbling blocks in antimatter technology, particularly in storage, these seem very likely to surface first in appropriate normal matter analyses and experiments accomplishable in Phase A.

In short, the ability to predict with confidence the basic feasibility of antimatter technology--go or no-go--should be largely accessible by Phase A end.

EXPANDED PHASE A, YEAR 1 DEVELOPMENT PROGRAM

- 7-12 month effort; focus on production, compact storage issues
  - ~$1/2 to 2 million (depends on scope)
  - Survey areas, critical experiments; propose experimenter/team choices
  - Seek national level team involvement

- Key outputs:
  - State of knowledge
  - Critical experiment definition (emphasize relevant normal matter research)
  - Identify possible showstoppers (fundamental?; circumventable?; alleviating approaches?)
  - Define early "demo" possibilities
  - Rough out schedules/costs for whole Phase A
  - Provide opportunity for first go-no go decisions

- Production issues—emphasize:
  - Factory scoping considerations
  - Confident prediction of basic process efficiencies
  - Validate issues, needs, objectives of Phase B mini-factory

- Compact storage issues—emphasize:
  - Survey, analysis of possibilities and alternatives (widest possible range)
  - Detailed definition of normal matter experiments
  - Establish conditions for successful pre-storage to final storage transition
  - Role of "portable" $\overline{P}$ traps for experimental purposes
  - Tentative selection of experimenter/experiment teams — scope, location
  - Get selected critical experiments underway

- Seek, where possible, for production/storage issues:
  - Initial judgments of prospective developmental suitability of selected concepts

A more detailed outline of the activities appropriate for the first year of Phase A are shown in the chart.

By the end of year 1, enough analysis, screening of concepts, isolation of real sticking issues, definition of experiments to resolve remaining uncertainties and ambiguities, and a generally better feel for the possibility of exploiting antimatter should permit focusing on several alternative outcomes concerning antimatter promise:

1. Attractive concepts seem reasonable, critical experiments can be formulated and gotten under way, and useful goals seem settable in principle. In this case, the full Phase A program would be undertaken.

2. Nonfeasibility, in principle or to achieve any reasonable applications goals, would be demonstrated. In this case no Phase A need be formally undertaken.

3. An intermediate position, where enough imponderables and uncertainties remain so as to make any or all of the remainder of Phase A perhaps interesting to pursue, but with no particular sense of urgency or coherence.

Rand feels that the year 1 effort is vital to select the subsequent path in a reasoned way, and has repeatedly urged that this initial effort be undertaken. It seems likely that enough interesting physics and engineering research paths could turn up to constitute a major attraction for a very broad cross-section of the physics and engineering community for the rest of Phase A and beyond.

From the point of view of overall initial physics and engineering interest, consideration of storage issues, particularly, would seem to have substantial appeal, with virtually every experiment initially performable in a high grade academic or industrial laboratory setting. This is one of the factors which strongly influences our opinion that progress in assessing the feasibility of exploiting annihilation energies can be substantially faster than much popular wisdom supposes.

ANTIMATTER PHYSICS, ENGINEERING INITIAL RESEARCH EMPHASIS

- Essentially *every* application will depend on
    - Production at reasonable efficiency and scale
    - Compact long term storage
- Warrants major attention initially on production/storage issues
- If antimatter is economically (application-sensitive) available
    - Potential application spectrum is very broad
    - Foreseeable roles in orbital propulsion, power, weaponry apparent
    - Availability likely to make other roles emerge strongly
- Some application paths can embody common technologies
    - E.g.: propulsion, power generation ←→ efficient heating of working fluid
- Recommended next steps in planning research
    - Major focus on survey, scoping of production/storage considerations
        - include critical experiment needs
    - Complementary emphasis on applications areas:
        - complexity/payoff issues
        - production scale implications
        - when antimatter use becomes operationally, economically attractive
        - generic technology threads; unique technologies
    - These next steps are prudent, low cost investments for the future

SUMMARY

- Consider use of antimatter

- Much basic science, engineering information known

- Phenomenology has potentially attractive features

- Premature currently to say system feasibility, utility unequivocal

- Steps needed to assess promise are well-defined

- Crucial steps, decisions can be taken in early study, experiment program

The main themes of this Note are recapitulated in the next two charts. While there are wide disparities of view on the probable time scale for developing antimatter technology, the case can be made that even if the technology should not be forthcoming before the next 20-30 years it is still prudent to carry out appropriately dedicated research in this area.

Our position can perhaps be put in the general terms: We do not now have a very confident time scale for the development of any level of antimatter technology. However, a much better perception of what shapes that time scale, and the steps which would be needed to realize a compressed time scale, can be obtained by carrying out a Phase A RDT&E program as outlined. Especially important would be the first year effort discussed earlier. In this way, one would progressively formulate and define the consecutive steps needed or desirable for an "antimatter industry," i.e., the relatively massive scale reflected by the culmination of Phase C. There are then a number of decision and commitment steps preceded by relatively less costly efforts.

This recommendation is based on what occurs to us as the real possibility that antimatter might well become a most important means for a portable energy store economically adapted to a broad range of applications--and at a time scale not wholly incommensurate with other past and current practical energy developments. Conduct of RDT&E in this field may itself produce major advances in a great many related fields. While today no one (neither skeptics nor enthusiasts) can confidently predict the outcome of Phases A, B, or C, enough appears to be known to formulate programs improving our confidence of getting actionable outcomes. The investments needed for these programs seem reasonable in light of the increasing levels of information gained, and the understanding achievable on whether and how antimatter technology might be developable.

The appropriate final message to be conveyed can be briefly stated:

1. We don't know enough today to decide whether antimatter applications are realizable in a reasonable time or are excessively long range.

2. We do know how to go about progressively improving our information base to make much more confident assessments.

3. If production at reasonable scale and cost and suitable storage are achievable, many applications seem attractive.

4. The investment to take the first assessment step (Phase A, year 1) is about 10^6; the full Phase A investment, contingent on a go from year 1, is about 10^8.

5. Understanding antimatter technology is prudent, and worth such investment.

6. The research efforts involved span so broad a range of basic interests that creative scientists with innovative ideas will find virtually unparalleled opportunities for novel and exciting science.[6]

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[6]A number of these opportunities are additionally discussed in a forthcoming publication by R. Forward.

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